research review

OFFICE OF AEROSPACE RESEARCH

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BASIC RESEARCH AND THE DEPARTMENT OF DEFENSE

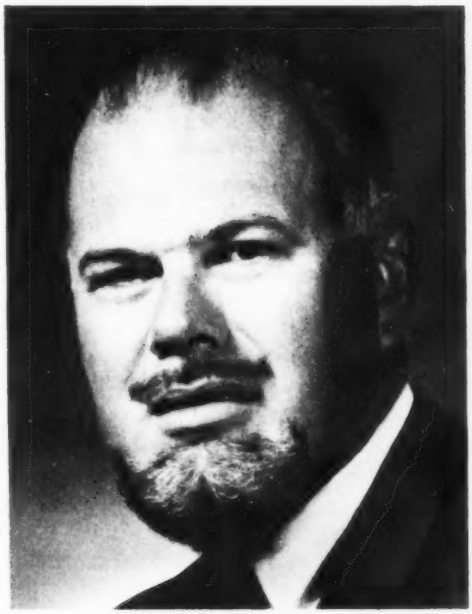
DR. HAROLD WOOSTER, Director of Information Sciences, AFOSR

A staff article in the March 1967 Reader's Digest, "The Great Research Boondoggle" by William Schulz, talked about $16 billion of Federally subsidized research and development. I trust the Reader's Digest -- if only to tell me what the public is interested in -- and $16 billion seems a big enough sum to be worth talking about, even in Texas.

I must confess that I have a little trouble in visualizing even $1 billion. I can see the $3 million my Directorate spends all too clearly, and can even pretend to understand the $35 million or so which my organization, the Air Force Office of Scientific Research, spends each year in buying basic research for its parent organization, the Office of Aerospace Research and, through them, for the Department of the Air Force. But I start blurring somewhere on this side of a billion dollars, so I made up this little chart: *(See chart on p.3.)*

Having gained some idea of what a billion dollars is like (and of my own importance in the Federal Government), I'll get on with my topic. There are three things I would like to discuss:

1. What is basic research?
2. Why is basic research a Good Thing for the Federal Government to support?
3. Why is basic research a Good Thing for the Department of Defense to support?



Dr. Harold Wooster

1. What is basic research?

The National Science Foundation defines basic research as "the search for an under-

standing of the laws of nature without regard to the ultimate application of the results."

We in the Department of Defense operate under a definition of "research" as "those efforts directed toward the increased scientific knowledge of natural phenomena and environment and toward the solution of scientific problems that have no direct military application."

Also, in industry, basic research is defined as "original investigations for the advancement of scientific knowledge that do not have specific commercial objectives, though such investigations may be in fields of present and potential interest to the reporting company."

Note that all three of these definitions share one common aspect: they lean heavily on the motives of those conducting and those sponsoring the investigations. And I don't have to tell you that motives are awfully tricky things to deal with: it is relatively easy to find out what a scientist is doing, but almost impossible to find out why he is doing it. A piece of elaborate, highly developed scientific apparatus may be used for routine testing, or for investigations into the fundamental properties of matter--but the clean white lab coat beloved of television commercials usually only shows that the scientist was expecting visitors!

Another problem. People, scientists or not, want love, sex, power, prestige, freedom and money, in various proportions depending on the person. Scientists typically put more emphasis on prestige and freedom than other people do. And prestige, in science, comes from doing basic research. This is not money prestige, mind you. The top scientific salaries in the country are, I believe, the $100-

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JACOB SEIDEN
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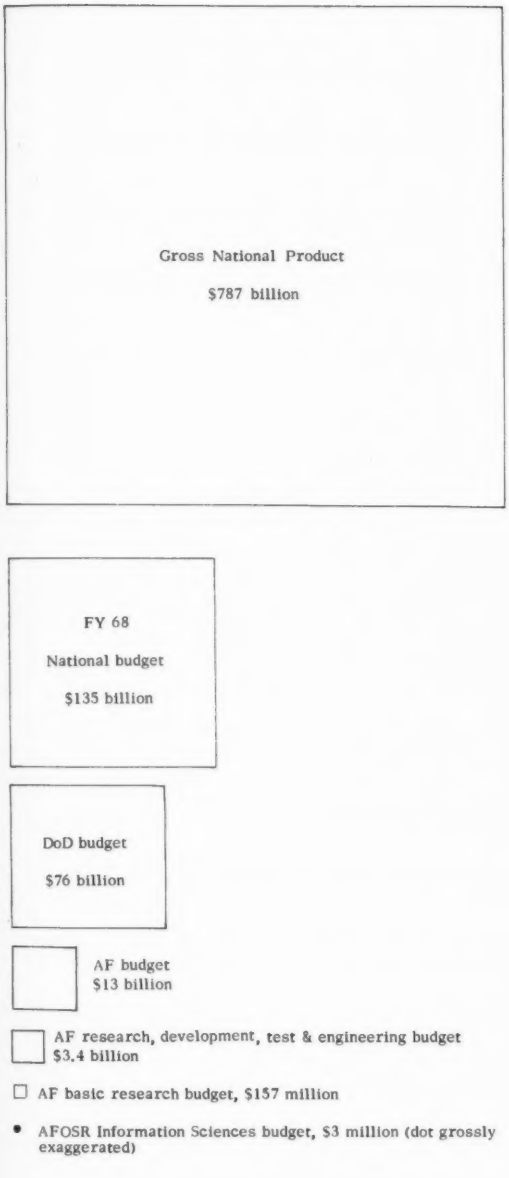

thousand Albert Einstein professorships in New York State, which actually pay the holder about $50 thousand a year. The average scientist is a salary earner, not an entrepreneur; he takes gambles with his science, not his money. He expects a comfortable middle-class income, with a ceiling of perhaps $25 thousand to $30 thousand a year (the median is a lot closer to $12 thousand). The prestige stems from appointments to important committees, to professorships in better schools, and by awards from his professional societies. And all this comes about through publication of the results of basic research. So the average scientist would prefer to work on basic, rather than applied, problems and sometimes to call what he is doing "basic research" even if someone else, doing more "basic" work, might call his research "applied."

In many ways, doing basic research is like drilling for oil. You make your best guesses as to where to drill, and keep plugging away until you hit something or decide to quit. Nobody wants to drill a dry hole; but it's one of the risks of the trade. And there's no guarantee that someone else can't pick up where you have already drilled and, by drilling a little longer, make a strike.

The qualities of a successful research man (Editorial, Chemical & Engineering News, 27 February 1967, quoting Dr. Vladimir Haensel, vice president of Universal Oil Products Company) are those you'd like to hire in a successful oil prospector--a thorough background in fundamentals, a good sense of reasoning, insatiable curiosity, the ability to make decisions, and perseverance. There is one major difference, though. With a well, you know immediately whether drilling it was worth the money. On the other hand, even a very good piece of research may take decades to pay off. And these days you have to dig deep to make a strike. You can still drill for a barrel-a-day result in a well-explored field; but the real payoffs take deep drilling in unexplored fields.

Basic research, by whatever definition you may use, took no more than 13 percent of the FY 67 Federal research and development budget (Federal Funds for Research, Development and Other Scientific Activities, Vol XV, NSF 66-25) so that the $16 billion the Reader's Digest talks about shrinks to no more than $2.1 billion, or slightly more than 1.5 percent of the total Federal budget. American industry, by way of comparison, spends about $607 million for basic research (Basic Research, Applied Research and Development in Industry, 1965, NSF 66-33)

which is about 4 percent of the $14.2 billion spent on industrial research and development. With the exception of the aircraft and missile industry, where basic research is financed almost equally by the Federal government and company funds, this industrial basic research is financed with company funds. And the industry which leads all others in the proportion of funds devoted to basic research, 15 percent, is petroleum refining and extraction! Any company in a reasonably competitive field--say petrochemicals, or pharmaceuticals or electronics--which spends only 1 percent of its sales on research can't be planning to stay in business for very long.

So perhaps those of us concerned with managing the basic-research programs of the Federal government have other than purely selfish reasons for wanting to see the Federal basic-research budget increased--the United States lives in a competitive environment, too, and I hope it's around for a long, long time.

2. But why should it be the Federal Government that sponsors basic research?

I should begin this section by pointing out that I am a great believer in the free-enterprise system--more so than, apparently, most of the industrial representatives who show up in my office seeking research support.

The only place where science and the useful arts are mentioned specifically in the Constitution is in Section 8 of the First Article, which gives the Congress the power to "promote the progress of science and the useful arts" by, very specifically, "securing for limited times to authors and inventors the exclusive right to their respective writings and discoveries." Since, by practice if not by definition, a basic law of nature cannot be patented, but only its application--this doesn't do much for research, whatever it may do for development. And since I am currently involved in the revision of the copyright law, I can assure you that whatever it may do for authors and publishers, copyright doesn't do much for science!

I'm afraid, then, that we have to fall back on the Constitution's Preamble--"We the people of the United States, in order to.... provide for the common defense, promote the general welfare, and secure the blessings of liberty to ourselves and to our posterity, do ordain and establish the Constitution for the United States of America."

I do not think that it is necessary to make a case for Federal support of basic research in the grandiose tones used by some of the spokesmen for Big Science. (The following quotation is via "Developments in Federal Policy Towards University Research," Harold Orlans, Science, 10 February 1967, pp. 665-668). It may or may not be true, but I think it hardly necessary to say, as Victor Weisskopf (Bull At Sci 21, No. 4, 1965) does in speaking of the new 200 BeV accelerator, that:

"The value of fundamental research does not lie only in the ideas it produces. There is more to it. It affects the whole intellectual life of a nation by determining its way of thinking and the standards by which actions and intellectual production are judged. If science is highly regarded and if the importance of being concerned with the most up-to-date problems of fundamental research is recognized, then a spiritual climate is created which influences all other activities."

I don't want to knock this approach. It certainly works beautifully in selling encyclopedias, or $600 sets of Great Books, to people who somehow think that owning books is as good as reading them. But I'm certainly not going to say that basic research should be supported because it is, according to Warren Weaver, "a supreme adventure of the human spirit." I tend rather to agree with Ivan Bennet of the Office of Science and Technology who says (quoted via AFCRL Newsletter Supplement, 27 January 1967): "Science can no longer hope to exist, among all human enterprises, through some mystique, without constraints or scrutiny in terms of national goals, and isolated from the competition for resources which are finite... Unless we are prepared to examine our endeavors, our objectives, and our priorities, and to state our case openly and clearly, the future will be difficult indeed."

There are good hard questions--the kind every industrial research manager should be

prepared to ask himself, and answer to his management--concerning what kinds of research should be pursued, in what scientific fields, and how much should be spent. At least five Congressional committees and subcommittees are engaged in asking just such questions: the House Committee on Science and Astronautics, the Senate Aeronautical and Space Sciences Committee, the Government Research Subcommittee of the Senate Government Operations Committee, the Research and Technical Programs Subcommittee of the House Government Operations Committee, and the Science, Research and Development Subcommittee of the House Space and Astronautics Committee, not to mention the Senate and House Committees charged with overseeing the budgets and activities of the separate Departments and Independent Agencies.

I think the fundamental premise, that the Federal Government has a responsibility to support basic research under the general-welfare clause of the Preamble, can be defended easily on three grounds:

(1) The more basic the research, the more general its applications. The results of basic research transcend state and national boundaries.

(2) Basic laws of nature cannot be patented. The discoverer may reap certain tangible and intangible benefits (but the Nobel Prize, the top prize in science, pays only $40 thousand, admittedly tax free, and the odds against any given scientist winning a Nobel Prize are 1:10,000). Money which pays taxes to keep the country going is made by those clever enough to apply these basic laws of nature.

Let me give you an example. The laser is being used these days for everything from drilling James Bond through the crotch (in Goldfinger) to drilling holes in diamond dies for drawing copper wire and to performing the most delicate of opthalmological surgery, reattaching retinas.

The Air Force Office of Scientific Research sponsored the basic research by Dr. Charles Townes which led to the invention of the laser. We spent, perhaps, $100 thousand in supporting the research. Dr. Townes shared the 1964 Nobel Prize in physics with two Russians who made the discovery independently--so he probably received $20 thousand after the split. According to an article in Chemical and Engineering News, 27 February 1967, $150 million will be spent on lasers in 1966, growing to perhaps $500 million to $1 billion in 1970. Granted, the Federal government is spending 70 to 75 percent of this money. But, and I think this is even more important than the over-all figure, this includes $29 million in commercial sales of hardware in 1967. George Stephan, marketing manager for lasers at Westinghouse, who I assume is no more optimistic than most marketing managers, predicts that this figure will quadruple by 1970 to $110 million.

So in this special case--and I'll admit that it's a very special case indeed--the investment of $100 thousand by a Federal office in supporting basic research, plus the contributions of American industry in developing and applying this idea, have together created a new source of industrial and tax income.

At least as far as commercial applications go, most of the holes drilled in basic research are very dry indeed. But I think it's just good business for us to pay for drilling the dry holes, and then let industry move in to exploit those with a practical pay-off.

(3) There are certain large research tools and facilities which are just too expensive for private industry, or any state or combination of states to build. The best-known, of course, are the giant accelerators of the Atomic Energy Commission; but we of the Air Force Office of Scientific Research have a few of these of our own, such as the National Magnet Laboratory at MIT, which has cost us perhaps $12 million to build and operate. This facility, unique in the Free World, can create and sustain larger magnetic fields--up to 250 kilogauss--than have ever been possible

before. This Laboratory is a national resource already making important direct applied contributions to the defense effort. It would have been impossible to build without Federal financing.

3. But why the Department of Defense?

Those of us who manage basic-research programs for the Federal Government must compete for our funds not only with other Government agencies, but also within our own DoD organizations. Within DoD, as in any other large organization, we must stress the importance of our particular activity within the over-all organization, and plead for our fair share of the budget. This has been going on in the Air Force at least since 1949, when a special committee of the Air Force Scientific Advisory Board, headed by the late Louis Ridenour, pointed out that Air Force research and development could not be maintained at the highest level of competence without being closely related with the general research efforts of the Nation's universities, and recommended that a fraction of the Air Force R&D budget be consistently assigned to contracts with educational institutions in broad general fields on problems which, without being directed towards definite goals or applications, were of definite interest to the Air Force.

It took a lot of faith in basic research to set up AFOSR back in 1955. Since then we've been able to come up with works to justify this faith. I've already mentioned our support of the research which created the laser, and our National Magnet Laboratory. We can also show that:

(1) AFOSR research in probability and statistics has led to direct improvements in such fields as communication theory, reliability theory, and weather forecasting.

(2) Improved education-and-training-methods research was started with our support of the first Symposium ever held on automatic teaching, back in 1958. This has had a very significant impact on teaching machines and on military education and training techniques in general.

(3) AFOSR cosmic-ray-physics studies have made valuable contributions to knowledge of the space environment. These studies involve the world's highest cosmic-ray laboratory at Mt. Chacaltaya in Bolivia, and an antenna array covering 100 square miles, the largest ever built.

(4) Seismology research, managed by AFOSR and funded by ARPA, has provided significant contributions in recording instrumentation and techniques for studying seismic-source properties and nuclear detection.

(5) AFOSR combustion research has had broad impact on such important areas as fuels and oxidants for high-energy missile propulsion, and combustion stability in operating rocket engines.

(6) Information-handling-and-retrieval studies supported by my own Directorate have led to such useful systems as the computerized management control data systems utilized by various Department of Defense agencies (our Agency for example was, to the best of my knowledge, the first Federal organization to install a computer-based system for handling research contracts some eight years ago--something which several major agencies have still not figured out how to do!)

(7) The phenomenon of favorable three-dimensional interference has been used by Grumman to design the air inlets for the F-11F-1F aircraft, resulting in a considerable increase in the power available.

(8) AFOSR-sponsored research at Columbia University on chemical reactions in the tail flame of a high-intensity arc led directly to a novel process for producing uranium monocarbide, which has been used commercially to produce this compound in many-ton lots for use in nuclear reactors.

I am sure that my friends in the Office of Naval Research, and in the Army Research Office, can show equally great benefits to their respective services.

But this is only part of the problem--our day-to-day and fiscal-year-to-fiscal-year

fight to justify our budget within the Air Force.

Very properly, the entire national budget for basic research is viewed as an entity by the Bureau of the Budget and the Office of Science and Technology. This involves now 16 Federal departments and agencies that conduct or support basic research, although only 5 of them are scheduled to obligate 90 percent of the total funds from 1965 through 1967. These Big Five are the National Aeronautics and Space Administration, the Department of Health, Education and Welfare, the Department of Defense, the Atomic Energy Commission, and the National Science Foundation. Of the remaining 10 percent of the funds, half are expected to be obligated by the Department of Agriculture, and the rest by the Departments of Commerce, Interior, Labor, Treasury, and Housing and Urban Development, the Civil Service Commission, the Smithsonian Institution, the Veterans Administration, the Office of Economic Opportunity, and the Small Business Administration.

There are considerable differences among the agencies in the levels of support provided basic research. In DoD, basic research represents only 4 percent of the total agency R&D budget for 1966; in NASA only 12 percent; and in the AEC, 23 percent. For HEW the share is 33 percent and for NSF 99 percent.

In the 1960's, NASA assumed the leadership in basic-research support. In FY 67, it was scheduled to account for 31 percent of all Federal basic-research effort. However, this figure was slightly (!) padded by the inclusion of the costs for expendable equipment in the forms of launch vehicles, spacecraft, and instruments used to obtain interplanetary data.

During the decade of the 1950's, DoD was the main Federal supporter of basic research, and AEC was second. But in recent years "civilian" organization programs as a whole have been receiving more basic-research support than the military organization programs. Since 1963, HEW has been second in basic-research funds, largely the result of efforts in the National Institutes of Health (administered by the Public Health Service), which sponsors a host of programs in the life sciences. AEC and DoD, now third and fourth, respectively, have grown only slowly in the 1960's, while NSF, still in fifth place, has shown the fastest growth.

Most of the postwar policies and patterns for the Federal support of basic research were established in 1946 with the formation of the Office of Naval Research. When the National Science Foundation, headed by the former Chief Scientist of ONR, was established in 1950 with the original mission of being the sole support of basic research in the Government, an agreement was soon reached with the other agencies that NSF's role was to encompass the support of "uncommitted" or basic research not tied to specific missions or objectives. We in DoD, NASA, HEW and AEC were and still are supporting "mission-oriented" research--research directly or indirectly geared to accomplishing the missions of our agencies.

I don't feel that the research we do in the mission-oriented agencies is necessarily any less basic than the uncommitted research sponsored by the National Science Foundation. Granted, we have one extra series of hurdles. We not only have to make sure that it is good research--at the very least for our own professional satisfaction--but we also have to be prepared to justify its support on grounds of Air Force need and relevance. This is sometimes a nuisance, but I don't think that it's all bad. And there's a certain something to be said for keeping in close touch with the users of the product. Years ago when I was in industrial research, I enjoyed talking to the salesman at least as much as I did to the V.P. for Research. It wasn't enough to turn out something from the laboratory--you had to find out how it stood up in the field.

So, one always hopes that when the national basic-research money is divvied up, the fact that the Department of Defense has a certain professional competence in selecting and managing basic-research programs won't be overlooked or forgotten.

Let me make one other point. Much of my time during the past 10 years has been spent in trying to figure out better methods for handling scientific and technical information.

I assume that I may be one of those people Mr. Schulz (Reader's Digest, op. cit.) refers to as a Government publicist grandiosely speaking of a federally spurred "knowledge explosion." If so, I belong in very distinguished company, including the present Vice-President of the United States. But by the union rules, which even a Vice-President has to observe, you don't talk about a "knowledge explosion"; you talk about an "information explosion." And, if you look closely at the "information explosion," you begin to wonder what happened to it.

It has been legitimate to say--it's one of those numbers which require too much trouble to disprove--that the world's scientific literature is doubling every 15, 10, or 7 years. Let's assume, for the sake of argument, that the world's scientific literature does double every 10 years. At best, or worst, this would give us from 8 to 10 times as much literature to cope with in 1965 as we had in 1940. But, during that same period, the money for research and development spent by the Federal Government alone, to say nothing of the private sector, increased 200 times. So perhaps a better question would be to ask whatever happened to all that information we were paying for?

I feel very strongly that it is the duty of every investigator supported by public research funds to make a full and complete report of what he has or has not accomplished with these public moneys, just as I feel that it is the duty of the agency supporting his research to make sure that this record is publicly available. We in AFOSR, for example, encourage our investigators to publish in the open scientific literature, or in the report literature, as we and they see fit. Ours is one of the few agencies which takes the trouble and resources to publish complete bibliographies of all the research it has supported. Just to give you an idea of the scale, last year our investigators published some 3,000 items--books, journal articles and technical reports. We forward copies of all of these to the Defense Documentation Center, where they are made available without charge to the Defense community. And, since most of our research is unclassified, we make these reports available for sale to the general public, through the Defense Documentation Center and by the Clearinghouse for Federal Scientific and Technical Information, which Mr. Schulz refers to as a "brick-and-glass monument to government's obsession with research."

I was fascinated by Mr. Schulz's reference to piles of reports 15 feet high, since I have been unable to pile reports more than 2 feet high on my desk before they start falling over. I called the Clearinghouse to find out how they did it. They were as confused by Mr. Schulz's reference as I was. They do have 30,000 square feet of storage space--but this is filled with steel shelving, all of which is 7 feet 3 inches high. (This 30,000-square-foot figure, by the way, includes aisle space.) In addition, they have another 7,500 square feet of storage space used for bulk storage of very popular reports in palletized boxes--and the height of these piles is limited by the stroke of their forklift trucks to 11 feet!

Mr. Schulz did show commendable restraint in one aspect, however. If he had been striving for cheap sensationalism, rather than making a quiet scholarly study of Federal support of basic research, he would not have referred to "half a million" federally financed scientific reports. The average number of copies stored at the Clearinghouse is a lot closer to one and a half million!

There's one little problem about these numbers. You have to keep in mind the difference between the title of a report, which is like a parts number, and the number of physical copies of the report, which is like the number of identical parts in a bin under a particular part number. The half-million figure refers to the number of titles, or "parts numbers," in the Clearinghouse catalogs. The number of titles that they have in inventory--it's the difference between ordering from the Sears Roebuck catalog and going to one of their big shopping centers--is a lot closer to 75,000. One and a half million is the number of reports they have on hand under these "parts numbers."

The Clearinghouse is like Sears Roebuck in still another fashion. The only way to get a report from it is to ask for it and pay for it. Last year they shipped out over two million

copies of reports, at the rate of 5,000 to 6,000 reports a day. A 100 percent turnover of inventory per year isn't too bad a figure!

Earlier in the article Mr. Schulz becomes properly incensed about the Office of Economic Opportunity's retaining authority to prohibit publication of the results of a study it was sponsoring. He quotes an editorial from the Washington Post to the effect that "The public, which is paying for them, would like to see the adverse results as well as the favorable ones."

In our own experience at AFOSR we have found that we average about one report per 20,000 research dollars. This figure is probably on the low side. Even so, at this figure, the 500,000 titles in the Clearinghouse catalog represent the expenditure of some 7 billion of taxpayer's dollars. I hate to think of just how worked up Mr. Schulz would get if we in the Federal Government kept these reports to ourselves, and did not set up a convenient mechanism to enable the taxpayer to have access to the results of the research he has paid for!

I have tried to give you a cursory notion of what basic research is all about, and to suggest that perhaps what the Irishman is supposed to have said about whiskey, "All of it is good, but some is better than others," fits basic research, too.

I submit that it is not illegal and immoral (even if, in my own case, slightly fattening) for the Federal Government to support research; that we in the Department of Defense in general, and the Air Force Office of Scientific Research in particular, are not completely and hopelessly incompetent as research managers; and that if we in the Federal Government didn't make every possible attempt to make the results of tax-supported research readily available, within the considerations of National security, the Reader's Digest would really have something to write about!

(This article is based upon a talk given before the Dallas Salesmanship Club, Dallas, Texas, 9 March 1967.)

Discovery of a Theoretically Unexpected Phenomenon: Optical Excitations Produced by Very-Low-Energy Ions

JOSHUA H. ROSENBLOOM, Directorate of Engineering Sciences, AFOSR

INTRODUCTION

A surprising physical phenomenon has been observed. Light in the visible and ultraviolet range has been produced by bombarding gases (such as argon, xenon, nitrogen, etc.) with beams of helium ions of low kinetic energies. The emphasis here is on the word "low." In previous experimental investigations, optical radiation had been induced with ion beams limited to relatively high kinetic energies--above 1,000 electron volts (eV). Here just as copious radiation was attained with beams of kinetic energies as low as about 10 eV. This observation at low kinetic energies was incongruous and contrary to the current thinking of the physics community in the field of atomic physics. The experiments with low-energy beams reported here were carried out at the Columbia University Radiation Laboratory by M. Lipeles, R. Novick, and N. Tolk. (1), (2).

In a sense, in 1939, nuclear fission presented an analogous surprise to the physics community (of course, with different ramifications for mankind). By bombarding an atomic nucleus with particles of kinetic energy of a few million eV, physicists had been able to "chip off" one or two protons or neutrons. It was accepted that, to break the nucleus into larger chunks as in fission, would require then-unattainable energies of hundreds of million eV. Thus, physicists initially greeted with amazement the announcement that raising the energy level of uranium by a mere 6 million eV with the addition of a neutron was sufficient to tear the nucleus apart into large chunks.

The point to be underlined in this experiment is that fundamentally it was feasible to observe this production of optical radiation at low ion energies as early as 40 years ago. However, it seems that no such experiments were consciously planned. The radiation was first noticed incidentally by the Columbia group in an experiment with independent goals.

RESULTS

Figures 1(1), (2), and 2(3) are typical of the results obtained, respectively, by the Columbia group and by other investigators

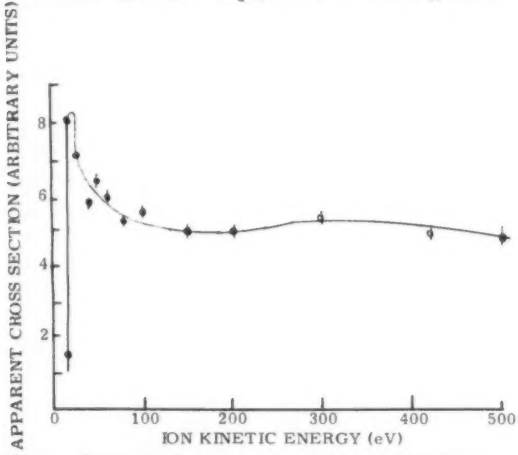

Figure 1. Optical excitation cross section for production of 4764.9Å line in argon gas by helium-ion impact (taken from Ref. 1).

during the past 40 years. Here we can observe the striking difference in the ranges for the kinetic energies of the incident beams at which radiation is emitted--a few eV with a maximum at 22 eV for the Columbia data compared to about 40 keV for typical data of the previous years.

The Columbia data relate kinetic energy and relative radiation intensity (actually plotted as excitation cross sections). The experimental situation underlying Fig. 1 involves the bombardment of argon gas by a beam of helium ions resulting in the emission of optical radiation of wavelength 4764.9Å. For Fig. 2, helium is substituted as the target gas and the resulting radiation is expressed in terms of quantum energy levels.

In the bombardment of a target atom by an incident helium ion, the kinetic energy of the

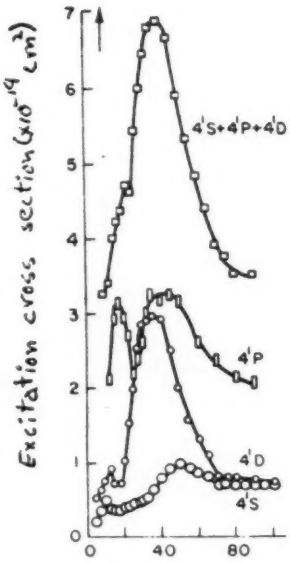

Figure 2. Excitation cross sections of He energy levels by helium-ion impact (taken from Ref. 3).

incident ion is decreased. The target atom makes a compensatory transition to an excited state with higher energy level. Actually, the incident and target systems interchange charge. Thus, for Fig. 1 the helium and argon systems interchange charge so that the interaction is given as:

$$He^{+} + Ar \longrightarrow He + Ar^{+*}$$

(here the asterisk * means an excited level)

Thereafter, the target system falls to a lower energy level with the emission of a photon. The information supplied by Fig. 2 is limited to the cross section for inducing excited energy levels - 4^1P, 4^1D, etc.--while Fig. 1 includes in addition the cross section for the succeeding emitted radiation of wavelength 4764.9Å.

HISTORY

Let us now take up the historical thread. As stated, Fig. 2 is typical of the results of investigations during the last 40 years. In

general, two characteristics dominated these investigations:

(1) The experiments were carried out mainly with incident beams of kinetic energies in the keV range.

(2) The optical excitation cross section, as a function of the kinetic energy of the incident beam, showed a single maximum in this keV range and decreased rapidly on either side of the maximum. (Exceptions with multiple maxima in the thousand eV range first were observed about 1962).

It became accepted that, if the kinetic energy of the incident ion were actually decreased towards a few eV, nothing especially interesting would occur. The optical excitation would just decrease to the threshold of the optical detecting devices--still near the keV range--and remain so for all lower values of the kinetic energy. This belief could not be based on a rigorous calculation. Such a rigorous calculation is too complicated even for the simplest case. The required support came from a qualitative analysis developed by Massey (4) in the form of an adiabatic criterion. However, no experimental verification of this accepted belief was ever consciously attempted.

The first recognition that the production of optical radiation was possible with the bombardment of ions of but a few eV came about incidentally in an experiment with an independent goal by this same group at Columbia. (5) The object of the experiment was to study the decay of He^+ in the metastable 2S state, i.e., the transition of the helium ion from the excited 2S state to the ground 1S state without interaction with external particles.

There is an especially interesting factor in this investigation. In the usual decay of an atom or ion between two energy levels, the mode of decay that predominates is the radiation of a single photon. In this case of the transition from the 2S to the 1S state, it was predicted that the dominant mode would be the radiation of two photons simultaneously with energies ΔE_a and ΔE_b, respectively, so that $\Delta E = \Delta E_a + \Delta E_b$ to maintain conservation of energy. Experimentally, the photons are detected by two photocounters, with the simultaneous two-photon decay being recognized when two photons are detected in time coincidence by the two photocounters, respectively. The decay was studied in a slow (12-eV-energy) beam of ionized helium containing varying percentages of ions in the excited 2S state.

The prediction was verified. The number of photon coincidences counted was closely related to the percentage of ions in the excited 2S state--the number of coincidences increasing (decreasing) with an increase (decrease) in the population of the 2S states. Then came the incidental observation which led to the work being reported here. With all the helium ions in the ground 1S state, i.e., none in the 2S state, no photon coincidences were detected. Similarly, no single photon counts should be expected since all the He^+ were in the ground 1S state--the lowest possible energy level--so that no transition to a lower energy could possibly occur. However, amazingly enough, a significant background of single photons was observed even with all He^+ in the ground state. Where could these come from? On tracking down the alternative possibilities, one conclusion stood out--the low-energy (12-eV) ground-state helium ions were interacting with the residual gas in the detection chambers containing the photocounters. This interaction raised the gas atoms to an excited state. In the transition of these excited states to a lower energy level, the observed photons were produced with a surprisingly high efficiency.

Thus, accepted scientific "folklore" had been disproved. A significant production of light was possible with impact from low-energy ions. This phenomenon was worth studying on its own merits.

THE EXPERIMENTS

This brings us to the major experiments to be reported here. A block diagram of the experimental arrangement is shown in Fig. 3. (2) The apparatus consists essentially of a helium-ion source, an intermediate chamber with cylindrical electrostatic lenses and microwave cavity, and the collision chamber including the target gases and the optical detection equipment.

The helium ions are formed by bombarding helium gas with electrons. The electro-

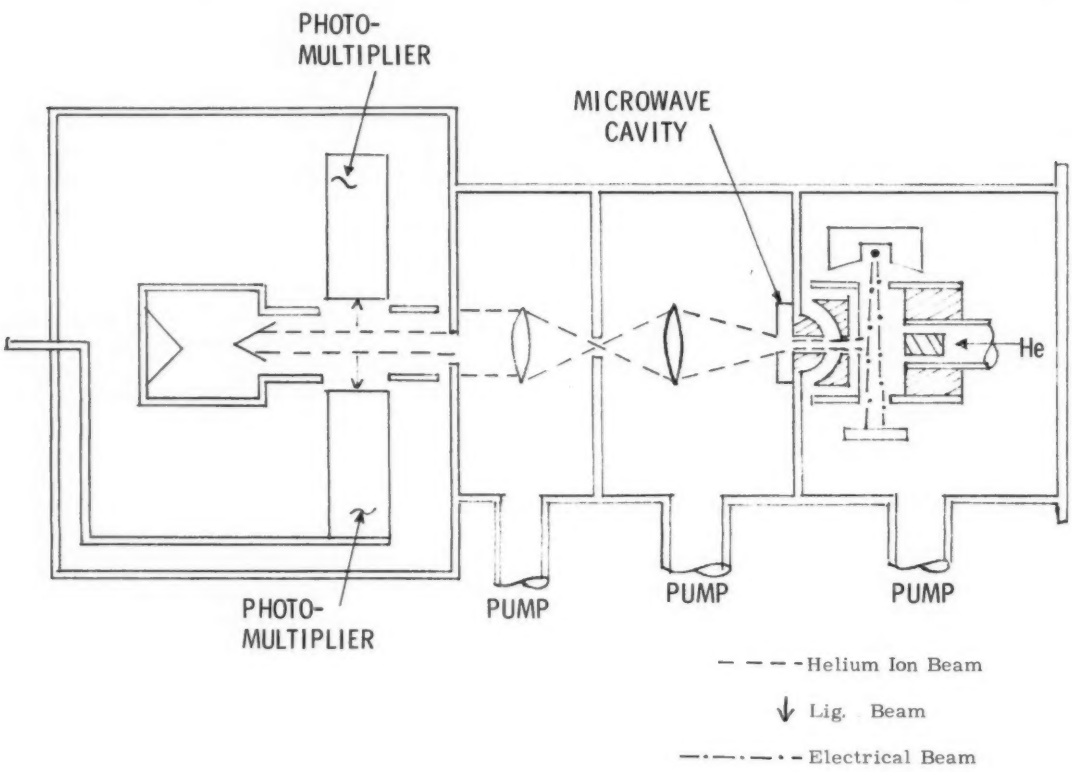

Figure 3. Block diagram of experimental arrangement to detect optical excitation by bombardment of gases with helium-ion beams (taken from Ref. 2).

static lens accelerates the ions, focuses them into a beam, and also rejects electrons emerging from the ion source. The ion beam has a current of $\sim 10^{-8}$ amp, and a kinetic energy of 10 eV (with a spread of $\pm$ 0.25 eV). The microwave cavity, when appropriately excited, quenches any excited 2S helium ions. The target particles include the rare gases as well as molecular gases. Optical radiation emitted along a portion of the helium beam passes through a filter or spectrometer, and the resulting single photons are counted by photomultipliers.

CONCLUSION

With significant excitation of light at low ion energies assured--in fact, even with well-defined maxima--two questions come to the fore. Why did Massey's adiabatic criterion discourage any expectations of significant optical excitations at ion energies of a few eV? Regardless of the expectations, why was this not verified experimentally?

To answer the second question plausibly, one should view it from the vantage point of a physicist indoctrinated with the success of the adiabatic criterion in the keV energy range, with no reason to doubt its predictions in the low eV range. He would expect only a negligible cross section at low eV. The experiment would measure and probably confirm a nearly negligible quantity. This is a difficult and not very exciting experiment--indeed, one to be shunned. An answer to the question concerning the apparent inadequacies of the Massey criterion will be treated in a future article.

In conclusion, the experiments reported here will significantly affect future studies of plasmas and the atmosphere. Previous studies considered excitation only from electrons or high-energy ions (in the keV range and above). With these new results, low-energy ion excitation may also be analyzed.

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cosmic radio waves

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One of the theories concerning the evolution of the universe states that all matter in it was at one time in a hot, highly contracted phase, followed by an expansion. It is further assumed that this expansion was uniform in all directions and that it is still going on. The red shift in the spectrum of a distant light source is, therefore, taken to be a measure of its velocity and, inferentially, of its distance. Distances based on this theory are called cosmological distances.

Recently, measurements in the radio-frequency regions of the electromagnetic spectrum have contributed to the evidence for this theory. Measurements of the intensity of the cosmic background radiation at several wavelengths support the idea that this radiation has a blackbody spectrum at about 3 degrees Kelvin. This is characteristic of a body emitting all wavelengths with a distribution appropriate to a particular temperature. The distinction between a blackbody source and other sources may be explained by the contrast of a red-hot iron in the blacksmith's forge with a neon light which emits some colors very strongly and is deficient in others.

The hypothesis that the background cosmic spectrum is a blackbody spectrum leads next to the thought that the universe is filled with blackbody radiation left over from the time when the universe was in a hot, highly contracted phase, i.e., a fireball. The next question then is whether or not this radiation is isotropic and homogeneous. Answers to this question would permit inferences concerning the nature of the expansion and its time scale.

At the Fourth Texas Symposium on Relativistic Astrophysics, 23-27 January 1967, R. B. Partridge and David T. Wilkinson (1) reported that the asymmetry of the background radiation (of 3 degrees Kelvin) at 3.2 cm wavelength was a few tenths of one percent at most. E. K. Conklin and R. N. Bracewell (2) of Stanford University, under AFOSR sponsorship, have also found that, at 3 cm wavelength, the background radiation is isotropic to less than one fifth of one percent.

The Stanford radiometer operated at a frequency of 10,690 Mhz with a band width of 100 MHz. The antenna was a 60-foot paraboloid mounted so as to point to the zenith.

Both measurements are consistent with the idea that the fireball had a temperature several billion years ago of some 10 billion degrees Kelvin, and that it has since cooled during the expansion to about 3 degrees Kelvin. They indicate also that there are no large inhomogeneities in the matter in the universe. An upper limit of $\pm$ 10 percent is placed on the variation of density large enough to extend over a few billion light-years.

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frozen state reactions

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Reactions commonly carried out in dilute solutions often proceed more rapidly when the solutions are frozen. This observation seems rather startling since it has been generally believed that chemical reactions are slowed down when the temperature is lowered. With support from the Directorate of Chemical Sciences, AFOSR, Dr. Richard Pincock of the University of British Columbia is studying the causes and extent of such apparently anomalous behavior.

The mobility of reacting molecules decreases as the temperature is lowered. However, unlike freezing a pure compound which is converted to a solid at the freezing point, the freezing of a dilute solution will sometimes initially result in crystallization of the solvent. When thermal equilibrium is attained, a liquid phase containing the solutes may still be present in the frozen mass at temperatures far below the initial freezing point. As the solvent crystallizes out, the reactant solute molecules are squeezed into minute pockets of liquid now containing higher concentrations of solute due to the decrease in volume of the liquid regions. Having been squeezed closer together, the reactant molecules collide more readily despite their decreased mobility. In his investigations, Dr. Pincock has observed accelerated rates of simple bimolecular reactions in frozen organic solvents and in frozen aqueous solutions.

Thus far some interesting general features for frozen systems have become apparent to Dr. Pincock and his co-workers. For example, the reaction of ethylene chlorohydrin with sodium hydroxide (forming ethylene oxide) in frozen solutions follows second-order kinetics with rates 1,000 times faster than supercooled liquid solutions at the same temperature (J.Am.Chem.Soc., 88, 4455, 1966). Fast reactions at temperatures as low as $-80^{o}C$ are illustrated by the oxidation of the iodide ion with arsenic acid in frozen aqueous solutions (J.Chem.Education, 43,358, 1966).

The enhanced reactivity of frozen-state reactions is largely attributed to the "concentration effect" by Pincock and other researchers in the field. However, in some instances the concentration effect does not tell the whole story. Other factors which have been suggested as contributing to the increase in reaction rates in frozen aqueous systems include molecular orientation by the ice crystals and catalytic activity on the surface of the ice. Dr. Pincock is considering these and other effects in his continuing study of reactions in frozen solutions.

This research on chemical reactions in frozen systems which is being done at the University of British Columbia could have far-reaching consequences. Although Dr. Pincock is concerned only with the chemistry involved, the results obtained are relevant to other scientific and technological areas such as the preservation of human tissues and organs, the storage of foods, and the question of whether or not life may exist at low temperatures during freezing conditions.

the Complexity of Isopoly Anions

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In the chemical laboratories throughout the world, a very small number of chemists are involved with studies of isopoly anions. Isopoly anions were not discovered in the Middle East, but therein lies a means for defining them. Consider a caliph who has a harem of four wives designated AO_4, where A = the caliph and O= a wife. An identical twin brother of the aforementioned caliph also has a harem of four wives, fortunately for us but possibly

*Dr. Rollins, Professor of Chemistry at the U.S. Naval Academy, wrote this article while on active duty as a reserve officer at the Directorate of Chemical Sciences.

unfortunate for him. We use the same designation--AO_4. At any rate, the two brothers combine (chemists would use the term, "condense") their harems. This process gives the product A_2O_7, which is sure to be an interesting combination. Note that one wife was eliminated. You may ponder on that. Now, the separate harems AO_4 correspond to simple ions such as SO_4^{-2}, while the combined harems correspond to isopoly ions such as $S_2O_7^{-2}$. The isopoly anions, like the joined harems, are complex combinations and are intriguing. This is why chemists study them.

Isopoly anions are formed by a condensation reaction when solutions containing simple ions such as MoO_4^{-2}, WO_4^{-2}, and VO_4^{-2} are acidified. (That well-known synthetic material which glamorizes the legs of women is also produced by a condensation process.) The acidified solutions may contain one, two, or several isopoly anions which are interrelated through complex equilibria dependent upon concentrations, pH (acidity), temperatures, time, and other parameters. The complexity of these solutions, coupled with the fact that the physical methods and techniques used to study them yield results which are at best indirectly achieved, have led to confusion and disagreement in this field. One investigator may study a solution which predominately contains one isopoly species and, as a result of diffusion studies, may postulate a 6-isopoly formula. Another investigator studying the same solution by light-scattering methods may report a 12-formulation for the selfsame ion. Determining the degree of aggregation of a particular isopoly anion by such methods is somewhat analogous to attempting to judge the number of spectators at a sporting event by the amount of noise they generate.

Dr. Lars G. Sillén's group in Stockholm, under AFOSR sponsorship, has been studying acidified solutions of the 3 simple ions mentioned above for the past 4 years. This group has used potentiometric (or emf) methods to investigate these solutions at 25°C and under constant ionic strength. The most controversial of the 3 systems studied is the tungstate (WO_4^{-2}, acidified). This system was researched over a wide pH (acidity) range, and all experimental results were consistent with the existence of only one isopoly species, paratungstate, whose formation in solution is given by the condensation reaction

$$6WO_4^{-2} + 7H^+ \longrightarrow HW_6O_{21}^{-5} + 3H_2O.$$

(The splitting out of the 3 H_2O molecules is analogous to the loss of the one wife from the harem.) The efforts of other solution investigators, e.g., Janer, Souchay, and Duncan and Kepert, corroborate the results of Sillén's group. On the other hand, Aveston recently reported that the acidified WO_4^{-2} solutions contain primarily $W_{12}O_{41}^{-10}$ ions (also called paratungstate).

Another group of investigators has studied solid paratungstates, i.e., salts which have been recovered from the solutions. In this camp there is disagreement also. Some contend that paratungstate is a W_6 species, but the majority report W_{12} formulations. In 1950, Lindquist performed an X-ray study of sodium paratungstate which he interpreted as demonstrating that the formula of the anion, in the solid, is $[W_{12}O_{46}]^{-20}$. It has been evident for some time that Lindquist's interpretation of his X-ray data is open to some serious questions. First, the anion is not compatible with the stoichiometry of the salt which, according to the analysis, contains 10 sodium atoms (per 12W atoms) and not 20 as would be required by Lindquist's formula. (The addition of 10 protons to Lindquist's anion would give $[H_{10}W_{12}O_{46}]^{-10}$, an ion compatible with the stoichiometry and his X-ray data). Second, the structure which Lindquist proposed is energetically unstable and lacking in the satisfying symmetry of arrangement found in other polyanion structures.

Very recently the author of this article performed a study (1) of the paratungstate anion which will be described below. Subsequent to his investigation, and as a result of his comments on it, Lipscomb re-evaluated the X-ray data of Lindquist. Lipscomb proposed that the paratungstate anion has the formula $[H_2W_{12}O_{42}]^{-10}$. The structure he offered differs considerably from that of Lindquist.

It was the author of this article who clarified the long-running controversy regarding paratungstates. Unlike all earlier investiga-

tors who studied either solutions or solids, he studied both solutions and solids. The initial breakthrough was made when he prepared the following new, pure, and recrystallizable salt: $(NH_4)_4Na[W_6O_{21+n}H_{1+2n}\cdot](8-n)H_2O$. This new compound and the pure sodium derivative, after being fully analyzed, were subjected to X-ray, potentiometric, and cryoscopic studies. The latter study, applied successfully for the first time in this area, showed clearly and unequivocally that the paratungstate anion, in solution, is a W_6# species as formulated in the salt above. The other studies cohere easily and unambiguously, and show that the same anion is present in solid paratungstates. The author's formulation for the paratungstate anion, $[W_6O_{21+nH_{1+2n}}]^{-5}$, is the same as that of Sillén's group after allowing for the possibility of constitutional water molecules.

An ultimate atomic position analysis of the new double salt shown above is under way employing X-ray techniques. Preliminary results indicate that the W_6# formulation is a possibility. Thus, a very specialized area of chemistry, in which disagreement has reigned for many years, has finally been elucidated.

Isopoly compounds such as the paratungstates may never compete with nylon; but studies in pure basic research such as those described above add to our understanding of (a) the forces bonding materials together; (b) why molecular structures are stable (or unstable); and (c) the condensation processes by which plastics and polymeric materials are produced.

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cloud distributions at supersonic-aircraft altitudes

AEROSPACE INSTRUMENTATION LABORATORY, AFCRL

A supersonic aircraft, at its cruise altitude of 40,000 to 60,000 feet, will not always enjoy a clear undisturbed sky while enroute to its destination. Just how often such an aircraft can expect to find clouds and precipitation at given supersonic altitudes is the subject of a study recently completed by Arthur J. Kantor and Donald D. Grantham of AFCRL.

Clouds and precipitation at Mach-2 speeds--even though encounters by aircraft will be relatively rare--pose special problems in supersonic operations. The leading edges of the aircraft surfaces can be damaged by precipitation droplets that would be harmless to subsonic aircraft; and even the otherwise moderate turbulence associated with clouds can be destructive.

The AFCRL statistical study of the distribution of high-altitude cumulonimbus clouds resulted from these considerations. Such clouds, composed of ice crystals or supercooled water droplets, occur more frequently in the stratosphere and are found at higher altitudes than was previously thought. AFCRL's U-2 meteorological research aircraft has frequently flown around and above cumulonimbus clouds as high as 62,000 to 64,000 feet, and radar precipitation echoes have been reported up to at least 70,000 feet at a number of radar sites.

In their study, Kantor and Grantham examined the daily records covering a 3-year period from each of the 31 weather radar stations of the Environmental Science Services Administration (ESSA) continental US weather radar network. The WSR-57 10-cm radars of the network can detect cloud peaks up to 70,000 feet within a 100-mile radius. From this ESSA-furnished data, the AFCRL scientists derived statistics on the diurnal, seasonal, and geographic distributions of high-altitude clouds between 35,000 and 70,000 feet.

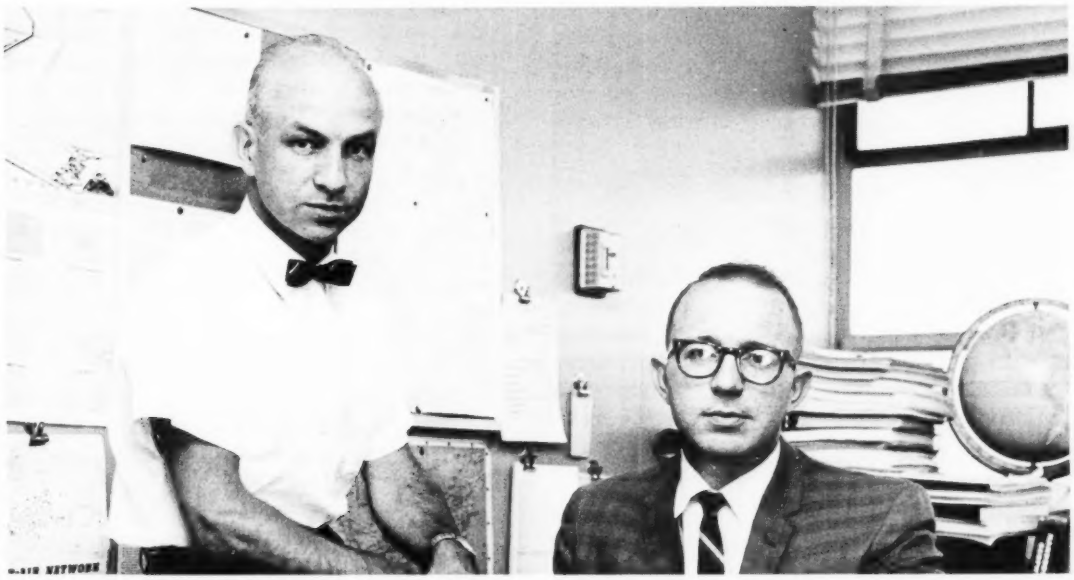
Although cloud distributions at high altitudes are similar to known thunderstorm distributions, the record of thunderstorm distributions does not necessarily reflect either the severity or the maximum altitude of high-altitude convective systems.

Results of the AFCRL study show that high-altitude cumulonimbus clouds are found most often in the Florida and Gulf Coast regions, with their occurrence decreasing northward. The high-altitude clouds are more numerous on the East Coast than on the West Coast. In all parts of the country, they occur most often in June, July and August. The month of most frequent activity tends to come earlier in the Midwest (May and June) than in the Gulf Coast regions (August).

From any arbitrary location in Florida during August, 55 percent of the time, one would expect to find within a 100-mile radius at least one convective cloud extending beyond 35,000 feet. Although convective clouds decrease in number with altitude, the number remains surprisingly high up to 60,000 feet. In the lower Mississippi Valley region in July and August, for example, one would expect to find, 4 percent of the time, at least one 60,000-foot system within a 100-mile radius.

In all parts of the country, high-altitude convective clouds are most likely to occur between 4 and 9 p.m., local time, each day. Secondary maximums, however, vary in different parts of the country. In the Kansas City region a secondary maximum occurs around midnight. A secondary maximum occurs at Key West at about 5 a.m.

Fragmentary radar observations for locations outside the U.S. indicate that U.S. data can be used as a first estimate of activity for land areas at corresponding latitudes around the hemisphere. Over some tropical regions somewhat greater frequencies are estimated to occur up to 50,000 or 60,000 feet.



Mr. Arthur J. Kantor (left) and Mr. Donald D. Grantham (right) are both research meteorologists with the Design Climatology Branch, Aerospace Instrumentation Laboratory, AFCRL.

Mr. Kantor received his Bachelor of Science in Meteorology from Pennsylvania State University in 1950, and attended both Rutgers University and MIT as a graduate student. Before coming to AFCRL in 1960, he was Meteorological Officer for the Irish Meteorological Service, Ireland. He also served with the U.S. Navy Hydrographic Office, was associated with the U.S. Weather Bureau as a New Jersey State Climatologist, and filled the post of Supervisory Meteorologist with the U.S. Air Force at McGuire AFB. At present, he is engaged in work in applied climatology.

Mr. Grantham was graduated from Ball State University at Muncie, Indiana in 1959 with a B.S. in Mathematics and Physical Science. He subsequently attended Texas A&M for his meteorological training. Prior to coming to AFCRL, he was Chief Forecaster at the Air Weather Service Detachments at Fort Sill, Oklahoma and Thule AFB, Greenland. Mr. Grantham is presently developing mathematical, statistical and graphical techniques for describing atmospheric parameters and the manner in which they vary in time and space.

Both Messrs Kantor and Grantham are members of the American Meteorological Society.

atmospheric density variations

UPPER ATMOSPHERE PHYSICS LABORATORY, AFCRL

To the upper-atmosphere physicist, the region between 80 and 120 km holds a special interest. It is within this region, at about 90 km, that atmospheric temperatures reach their lowest point--sometimes as low as -110 degrees C. It is here that the ionosphere is highly reflective. Also, it is within this region that atmospheric density increases rapidly and meteors vaporize, and where the enveloping plasma pocket first forms around re-entering spacecraft. High-altitude noctilucent clouds are found within this region and, at its upper altitude range, auroral activity begins.

On reference charts, in model atmospheres, and in textbooks, the various parameters of this region--density, temperature, and composition--are stated with a bold and unqualified authority that often masks the tentative and approximate nature of the data. Of the parameters of the upper atmosphere, densities are of basic interest. Density data is directly related to many other parameters of interest--temperatures, chemical-reaction rates, and composition.

Perhaps no other research laboratory in the world has made more measurements of upper-atmosphere densities than the Air Force Cambridge Research Laboratories. AFCRL, during the past 5 years, has instrumented well over 100 rockets expressly to make density measurements, and has used a variety of different measuring techniques. In a recent AFCRL report, "Variations with Season and Latitude of Density, Temperature, and Composition in the Lower Atmosphere" (AFCRL-67-0161), Dr. K. S. W. Champion reviews the data that have accrued in recent years, and redefines existing atmospheric models of densities between 80 and 120 km. He suggests that, in this altitude region, values may differ by as much as 50 percent in some cases from those found in the current basic reference document, The U.S. Standard Atmosphere, 1962.

The essence of the report is a single chart depicting mean seasonal and latitudinal density variations between 80 and 120 km. The chart consists of a series of curves showing seasonal density deviations in percentages from a mean value as a function of altitude at 4 latitudes: 15, 30, 45, and 60 degrees. The resultant plot has one curious feature, which occurs at an altitude of 91 km. At this altitude all the density curves cross at a mean value. Taking just 2 of the several curves as an example, we see that the 60-degree-latitude curves for winter and summer combine to form an X configuration, with the crossing point at 91 km. This is also the point at which all the other curves cross. Thus it can be seen that, where the density for a given latitude and season is greater than the mean at altitudes above 91 km (as it is for all the winter curves), after the crossing point the density is less than the mean for the altitude. The plot also shows that the higher the latitude, the greater the deviation from the mean density. The mean winter atmospheric density at 60-degree latitude at 120 km, for example, is more than 50 percent greater than the mean density for that altitude; at 80 km, it is 25 percent less. The summer curve for 60 degrees is roughly a mirror image of the winter curve, although the deviations are less at high altitudes.

Of the many high-altitude density-measuring techniques used by AFCRL, two have been used most extensively. One of these is an omnidirectional accelerometer contained in a free-falling sphere released from a rocket; the second is a free-falling balloon with interior corner reflectors to provide a stronger radar return as the balloon is tracked from the ground. Another method consists of releasing a chemical in the upper atmosphere and measuring its rate of expansion. From the rate of expansion the diffusion coefficient is calculated. If the atmospheric temperature is measured at the same time, density can be deduced.

laser DAMAGE

SOLID STATE SCIENCES LABORATORY AFCRL

A whole new domain of applications awaits the laser system that can handle high input power without destroying itself. But the rods, mirrors, prisms, and lenses of laser systems are all subject to damage when operated at high power. Even when the system is operated at moderate powers over a period of time, breakdown can occur through minor accumulative modifications.

Carl A. Pitha, in an AFCRL report, "Laser Damage: A Selected Literature Survey" (AFCRL-67-0137), examines the nature of the damage to laser systems and components. This AFCRL study is based on an analysis of reports in the literature on the breakdown of laser systems.

In spite of the rather massive amount of laser research going on everywhere, no one has paid much attention to a systematic analysis of damage-producing mechanisms. There is no compilation of data on various breakdown mechanisms in the order of relative significance. Everyone involved in solid-state laser research knows, of course, that the laser rod is a vulnerable component in the laser system, and that structural imperfections and impurities influence rod damage. But, from the standpoint of designing more damage-free systems, this knowledge isn't very useful. Structural imperfections and impurities in the laser rod and in associated glass components occur in a number of forms which are difficult to isolate, identify, and count. Progress toward damage-free systems, however, depends upon just that.

Damage to the rods, lenses, and prisms of laser systems usually consists of pits on the surfaces and bubbles and fractures that take place internally. Pitha, in his report, observes that the power level at which damage occurs depends on the composition of the specimens, the manner in which they were grown and prepared for use and, in crystalline specimens, on their orientation.

Mr. Carl A. Pitha is a supervisory solid-state physicist with the Purification and Growth Branch, Solid State Sciences Laboratory, AFCRL. He received both his B.S. (1946) and his M.S. (1947) in Chemistry from the Polytechnic Institute of Brooklyn. Before coming to AFCRL in 1957, Mr. Pitha was employed by the Niagara Chemical Division, Food Machinery Corporation, Middleport, New York and the Sylvania Electric Products Corporation, Woburn, Mass. Presently, at AFCRL, he is heading up research efforts relating to the growth and characterization of single crystals by flame fusion techniques. Mr. Pitha is a member of the American Chemical Society, American Mineralogical Society, and Sigma Xi, and the author of several articles.

Of the laser-damage problems considered, the one to which most attention has been given is: How does energy accumulate to the extent that damage results in laser components? Three primary mechanisms have been proposed:

a) Self-focusing, attributed to the ampli-

tude-phase perturbations of a plane electromagnetic wave in a nonlinear dielectric, creates converging "pipes" in which light and acoustic waves tend to concentrate. Little experimental work has been done on self-focusing in solids, in spite of the common occurrence of filaments of high energy in lasing elements.

b) Stimulated Brillouin scattering provides a means of coupling acoustic and optical waves in a manner resulting in the concentration of acoustic waves. Experimental investigations of this mechanism have been primarily limited to liquids. So far, it has not been proved directly to result in damage in solids.

c) Multiphoton absorption, the successive absorption of two or more quanta of light by the same atom, has been found to occur in ruby. In this material, it could represent a major source of damage, though no direct evidence demonstrating this has been presented. The operation of this mechanism is dependent on the characteristics of the electronic states of the electromagnetically active ion.

A secondary mechanism, microplasma generation, requires that a specimen locally reach a high-enough temperature to cause at least a small amount of ionization. The ionized material will absorb the laser beam and become hot enough to damage the material. Although this mechanism has been found to function on the surface, it has not been shown to take place within the body of a specimen.

THE SPICULE CONTROVERSY

DR. JOHN W. EVANS, Space Physics Laboratory, AFCRL

Early eclipse observers first saw the solar chromosphere as a rosy, thin arc around the silhouetted disk of the moon. Photographs of the chromosphere in white light give the impression of a continuous medium that begins at the photosphere (the visible surface of the sun) and thins out with increasing distance from it, becoming invisible as it merges with the solar corona at about 6,000 to 7,000 km outward from the photosphere.

When viewed in Hα, however, the chromosphere is seen to be highly inhomogeneous. It appears as a fur of bright spicules, little hairy spikes of gas, 5,000 to 10,000 km high, and less than 1,500 km thick projecting upward into the corona from an irregularly defined continuous layer.

These observations have given rise to a lively controversy among solar astronomers. Those at AFCRL's Sacramento Peak Observatory in New Mexico now believe that they have resolved the controversy--at least to their own satisfaction. The controversy arose out of the following questions:

Does the chromosphere consist wholly of a "forest of spicules," too numerous to be individually resolved at the lower levels, but with a few exceptionally high individual ones that are seen separately higher up? If so, the spicules should be surrounded by coronal gas at extremely low density and high temperature (about 1,000,000 degrees K). Or, are the spicules immersed in a continuous chromospheric atmosphere of relatively high density and low temperatures (about 10,000 degrees K)?

Proponents of the spicule forest theory point out that the theory accounts neatly for the low intensity of high-frequency radio noise from the chromosphere, and the double reversal of the H and K lines of Ca II on the solar disk.

In such reversals, spectroscopic records show a narrow dark line in the center of the strong bright line of these elements. The reversals could be produced in the spicules themselves if their density is sufficiently high (as the forest theory proponents contend), or they could be due to the absorption of light from the spicules as the light passes through a cooler interspicular medium. AFCRL astronomers have looked for spectroscopic characteristics that would resolve the question. They have found that the reversals are due to the absorption of light by the interspicular medium. Their conclusions

are based on the analysis of Doppler characteristics.

The spicules are a dynamic phenomenon, with mass velocities of 20 to 40 km per second. Often a large component of velocity lies along the line of sight, producing a substantial Doppler shift of the spicular line spectrum. The AFCRL spectra show these wavelength shifts very clearly. They also show that the dark reversed lines in H_α, H, and K do not share the Doppler shifts of the bright spicular lines. The material producing the reversal, therefore, is not moving with the spicule material. AFCRL astronomers have concluded that the dark line is due to a stationary interspicular medium at the temperature and density favorable for the absorption of H_α, H, and K. At heights above 6,000 km, the reversal fades out. Hence the lower spicules are immersed in a continuous chromospheric atmosphere, which interfaces rather abruptly with the hot tenuous corona at about 6,000 km.

The study of spicule spectra has also yielded the interesting fact that the spicules show Doppler line distortions peculiar to axial rotation. The spicules appear to ascend with material spiralling around their long axis.

Dr. John W. Evans is Director of the AFCRL Sacramento Peak Observatory at Sunspot, New Mexico. He is responsible for the direction of all activities at the Observatory, both scientific and administrative, and is currently studying the relation between fluctuations in magnetic fields, mass motions, and the optically visible phenomena related to solar flares.

Dr. Evans has a B.A. from Swarthmore College (1932) and a Ph.D. from Harvard (1938). Before joining AFCRL in 1952, he had been an Assistant Professor of Astronomy at Mills College; a supervisory optical physicist at the Institute of Optics, University of Rochester; and an Assistant Director of the High-Altitude Observatory at Boulder, Colorado. He is associated with a number of professional societies, and has published several papers. From 1948 to the present he has been a member of Commissions 9 and 12 of the International Astronomical Union.

radiation of volcanic heat into space

TERRESTRIAL SCIENCES LABORATORY, AFCRL

How much of the heat from an erupting volcano passes through the atmosphere and is radiated into space? Until recently there was no basis for valid estimates. However, a set of fortuitous circumstances involving a satellite, an infrared-instrumented C-130, a geological expedition arrayed with a variety of portable infrared sensors and, most important, an unscheduled erupting volcano combined to provide geologists with some invaluable data on volcanic thermal radiation.

The basic event occurred on August 19, 1966 when Iceland's new volcano, Surtsey, erupted again. For two and a half years the volcano had been relatively quiet. (This is the volcano which many will remember as first erupting at sea in November 1963, forming a new island 17 miles off the southern Iceland coast, and providing some rather

spectacular film footage for depicting the formation of the earth in the motion picture, "The Bible.")

By coincidence, a team of AFCRL, U.S. Geological Survey, and University of Michigan geologists was in Iceland at the time of the second eruption to conduct a routine geothermal survey--a survey of "hot spots" or temperature increases at the surface where heat from the earth's interior radiates through the mantle. Codirectors of the expedition were 1st Lt (Dr.) Richard S. Williams, Jr., of AFCRL, and Dr. Jules Friedman of the U.S. Geological Survey. A key expedition facility was AFCRL's C-130 aircraft.

Immediately after Surtsey's eruption, expedition members turned their attention to the volcano. Flights were made over it within a few hours after its eruption. A wealth of airborne and ground-based infrared data was gathered during the next few days. But it was only after Lt Williams had returned to AFCRL that the idea occurred to him that the eruption might have been detected by the infrared sensors aboard the Nimbus-II satellite. He checked with AFCRL's Satellite Meteorology Branch where data from the Nimbus-II satellite are recorded routinely on Automatic Picture Transmission (APT) equipment. Sure enough, he found that the erupting volcano had been clearly identified in the satellite data. Lt Williams then notified Dr. Friedman who made an independent check at NASA of Nimbus-II data. From the combined satellite, ground, and C-130 data, Lt Williams and Dr. Friedman estimated that 4 percent of Surtsey's heat in the 3- to 4-micron region was radiated into space. The other 96 percent was absorbed by the land, sea, and atmosphere.

First Lieutenant Richard S. Williams, Jr. is a research geologist with the Geotechnics Branch, Terrestrial Sciences Laboratory, AFCRL. He has a B.S. (1961) and an M.S. (1962), both in Geology and both from the University of Michigan. He also has a Ph.D. in Geology from Pennsylvania State University (1965).

Presently, Lt Williams is involved in tasks having to do with sensor imagery and landform analysis, remote sensing, planetary photogeology, and terrestrial landforms and environmental processes. He is a member of the Geological Society of America, American Society of Photogrammetry, and other professional societies.